THE
BOSTON MEDICAL AND SURGICAL
JOURNAL.

VOL. X.] WEDNESDAY, MAY 14, 1834. [NO. 14.

MEDICAL PROPERTIES OF THE KALMIA LATIFOLIA, OR BROAD-LEAVED LAUREL.

[Communicated by "W." for the Boston Medical and Surgical Journal.]

DEAR SIR,—The following is an account of facts, relating to the medicinal properties of the leaves of the Kalmia latifolia, which have come to my knowledge, and of experiments with them tried on myself.

At the time of commencing my experiments, I did not know that the article had ever been used medicinally, but had often heard of its being poisonous to lambs; and its effects on them are described by those who have seen them under its influence, as being like those of a narcotic poison. On the 20th of April last, I prepared an infusion of the recent unbruised leaves, ʒij. to a pint of boiling water, and began at 3 in the afternoon with ℥ss. of this. This infusion had very little of the sensible properties of the leaves, which are not at all strong, though considerable sense of acrimony is left in the fauces after chewing and swallowing a portion of a leaf. The leaf also contains considerable mucilage, but my first infusion contained none, and from its feeble sensible properties I did not expect any effect from it. Pulse at this time 63; six or eight beats above the usual standard—probably owing to a fuller meal than usual. Fifteen minutes past 3, took ℥j. Half-past 3, more tendency to perspiration than there had been. This might or might not be from the medicine. Forty-five minutes past 3, took ℥j. At fifteen minutes past 4, took ℥ij. Half-past 4, there being no decided effect, took all that remained, being about ℥vi. No other effect for some time but slight pricking sensations, such as I had often felt from heat in the spring, attended with increased tendency to sweating. At 5, began to be sleepy. At the same time there was a sensation of warmth through the epigastric region, which gradually increased, and with it the sleepiness. Went to sleep in twenty minutes, and slept till awaked, at twenty minutes past 6, when I was considerably dizzy, but took tea as usual. Pricking sensation in the skin considerably increased, and was most conspicuous in the scalp. This pricking sensation is peculiar, and I can compare it to nothing but the effect produced on the mouth by ess. peppermint, which is a sensation of coolness, with pricking by innumerable minute points at the same time. Sensation of great fatigue in all the muscles, especially when exerted. This first appeared in those of mastication, which were excessively fatigued by a very moderate exertion. Dizziness increased, and with it the sensation of fatigue. Soon there began to be a sensation of gradual contraction of the stomach, forcing its contents up into the œsophagus, and a feeling as of a ball rising in the throat, but without the least nausea. Vomiting began at about 7, without the least nausea, but with an action like rumination. At this time there was dimness of sight, which was greater in the paroxysms of vomiting, which recurred every three or five minutes for an hour. Considerable bile thrown up towards the close. Muscular prostration was such that I could barely walk across the room. The affection of vision amounted almost to complete blindness, when in an erect position. I had before for experiment taken Veratrum viride as an emetic, and the peculiar sensations in the skin and muscles, and the manner of vomiting and sensations in the stomach, were precisely the same from the two articles. The pulse was not examined till after vomiting. It was then extremely weak and creeping, the artery seeming slowly to contract and dilate like the action of an earth worm. Its frequency was 40 in a minute. I was at this time sitting in a chair. Skin cool, covered with cool sweat. Secretion of saliva more free than from an ordinary emetic, but not so profuse as from Veratrum. Sensations in the bowels as if there would be a cathartic operation, but none followed. Vomiting stopped at about 8 o'clock; but the cool skin, weak slow pulse, dizziness, and muscular weakness, continued till past 9, with little or no diminution. I then took, at two draughts, about a tumblerfull of good wine, the effect of which was immediate in relieving my symptoms, and in fifteen minutes I felt every way as well as I ever did. In the course of this experiment there was slight pain in the forehead. Very likely this in another person would have been considerable; but I never had any such thing as severe headache from any cause.

On the 21st I prepared an infusion of the bruised leaves with the same quantity of water. This had the sensible qualities of the leaves strongly, and was very mucilaginous, so much so that it was difficult to get the liquid without portions of leaves. At half-past 8 in the morning, took ʒj. of this infusion. At 10, felt slight dizziness and fugitive glow in different parts of the surface, with increase of sweating. I thought that there was some increase of urine during this experiment, and also in the first. Fifteen minutes before 11, sensations in the skin increased, with itching and more sweating. Fifteen minutes past 11, glow through the epigastric region, with feeling as though some new action was going on there. Fatigue of muscles, as in the first trial; my legs feeling, on going up stairs, as though I was struggling under the utmost weight they could support. Considerable sleepiness. Thirty-five minutes past 11, being then lying down, pulse was only 35 in a minute. It was, however, less weak than last night. At 12, some feeling of contraction at stomach, which, however, was not followed by vomiting. Ate dinner at half-past 1. Sleepiness continued till 4. The slowness of pulse also continued till after dinner.

Being in Albany in May, I saw Professor Tully there, and mentioned to him my experiments with this article. He said he had used it, and gave me the following written statement of his experience in regard to it. "While engaged in the practice of medicine in Middletown, in several instances I treated Rheumatismus subacutus with about one part of the tincture of Kalmia latifolia to three of the infusion. Both these preparations were in the proportions of ℥ij. Troy to a pint of the menstruum. The doses and quantities in the twenty-four hours I do not now recollect, but I made the effect the test of both. My doses always fell short of disquieting the stomach; and the production of a certain degree of vertigo and imperfection of vision was the test of the quantity in the twenty-four hours. The operative effects which I observed were a pricking sensation in the skin generally, moderate sweating, warmth in the stomach, relief of pain and watchfulness, then perhaps some somnolency, vertigo, and imperfection of vision. The complaint was generally relieved in from one to three days after commencing the use of the remedy. As my doses were not large, though repeated many times in the twenty-four hours, I never saw vomiting produced. I considered this article as analogous to the Rhododendron chrysanthum, as its effects are described in the books, and apparently of about the same power."

On my return from Albany, I prepared a tincture by macerating ℥ij. of recent cut leaves in a pint of alcohol. On the 4th of July, at half past 2 in the afternoon, began with ʒij. of this. Pulse at this time from 60 to 64. At 4, thought I perceived slight sensations of no very definite kind, yet differing from what I felt before taking the article. Took ʒij. more. In about half an hour slight glow felt in the stomach, with vertigo and pricking in the skin. I always sweat very freely at this season, but I think sweating was more free than is usual with me at the same temperature. Vertigo gradually increased. Fifteen minutes past 5, took ʒij. At 6, pulse 48, strength not affected, or only slightly affected. Fifteen minutes past 6, took ʒij. Half-past 6, considerable somnolency. Forty-five minutes past 7, pulse 38, weak and irregular; skin cool. All the symptoms increased. Uneasiness at stomach; not nausea, nor yet exactly the same feeling produced by the infusion. Somnolency very considerable; muscles excessively fatigued with slight exertion. Vision much affected, especially by darkness coming before the eyes every three or five minutes, and oftener if in an erect position. Could not go above three steps without being completely exhausted, and likely to fall from weakness and dizziness. Slight pain about the forehead. At 9, vomited some, with the same ruminating action as in my first experiment, after which I soon fell asleep and continued so till 5 in the morning. Soon fell asleep again, and continued asleep till waked at 7. Felt as well as usual, except slight uneasiness in the forehead, not amounting to pain. Whole quantity of the tincture taken, ℥j. It appears to be rather more powerful than the infusion, though probably the latter would be best in most cases.

On the 15th of January last, at 8 in the morning, took twenty grains of the powder of the leaf. It was not sufficiently fine. Pulse about 58 before taking it. Half-past 10, rather dizzy; pulse 48; slight sensations throughout, like what it produced before, but less of the pricking in the skin. 12, effect not increased; took eighteen grains more. 1, some glow in the stomach, and feeling like a ball rising in the throat. Dizziness increased; sensation of a cloud before the eyes at intervals. Muscles and skin affected as in former trials. Pulse 40, and very gentle in its action, as before. Sleepiness increased. 2 o'clock, just awaked. Pulse 37; disagreeable feeling at stomach, without the feeling of contraction there which attended in the first trial. 3 o'clock, pulse same; disagreeable feeling at stomach gone. Half-past 3, took forty drops of laudanum, upon which the effects rapidly went off. I presume that half the quantity of the fine powder would have had as much effect as did the quantity taken.

On the 27th of February I took twenty grains of a powder made finer than in the last instance, but still hardly fine enough. The effects produced were just about equal to what were produced by thirty-eight grains before.

March 2, began in the morning with a dose of ten grains of powder of Kalmia, intending to take it some days in the quantity of ten grains every four hours; but in two or three hours the effect of this dose was almost as great as from twenty grains three days before. I then took it in five grain doses, five times a day, for about three days. Sweating was decidedly more free than usual, and digestion better. There was considerable itching. Did not perceive any effect on any other secretion than that of the skin. I obtained an alcoholic extract by evaporating five and a half ounces of the tincture. Quantity obtained, ʒij. This has the sensible properties of the article strongly, but probably it is but little stronger than the powder.

On the 6th, I prepared an infusion by bruising the leaves, tying them up in a coarse cloth, and pouring boiling water on them. Precisely the same effect produced by ℥iss. of this, as in former trials. This infusion, however, was not mucilaginous at all.

I have mentioned the similarity, or almost perfect identity, of some of the effects of this article, to those of the Veratrum viride as experienced on my own person.

On the 20th of December, 1832, I took two grains of the extract of Veratrum viride, prepared by inspissating to a dry powder the expressed juice of the root, dug in the fall, after the death of the top. Slight glow in the stomach followed, much the same as a dose of any pungent substance would produce, but no other effect. In half an hour took four grains. No other effect for half an hour than an increase of this feeling. Then began a feeling as though the stomach were very slowly contracting on its contents, and forcing them into the œsophagus, producing a sensation as of a ball about the top of the sternum. Sensation in the epigastric region as though a full dose of calomel were operating. About three-quarters of an hour after the last dose, vomiting was produced, without effort or the least nausea. Peculiar sensations in the skin. Repeated vomiting followed, with some hiccough; considerable bile thrown up. Great increase of secretion of saliva. Skin become cool, much dizziness, cloud before the eyes, great prostration, considerable fatigue of muscles. Pulse very weak, small and soft, and only 34 in a minute. Sense of contraction at stomach increased and become very disagreeable, still without nausea. The cloud before the eyes was chiefly present during vomiting. About three-quarters of an hour after the first vomiting, as vomiting continued by turns and other symptoms did not diminish, took ʒss. of laudanum, and in twenty minutes forty-five drops more. Relief very soon followed, and I went to sleep. Slept quietly till next morning, when I awoke, feeling better than I had done before for six months.—About 8 o'clock, morning, took three grains more. No effect for two hours, when there began to be queer feelings about the head. Sensation of cramp in the gastrocnemii followed, with inability to exert them. There was no contraction in them, but they were for the time almost paralytic. This feeling went off in about an hour. At fifteen minutes past 12, took three grains more. The same sensation in the gastrocnemii returned, and the muscles of the forearm were also affected in the same way. Vomiting followed in forty-five minutes, attended by all the symptoms which attended the night before, but in rather less degree. Took forty-five drops of laudanum, which relieved me soon.—At 11 at night, took two grains more, not expecting it to produce vomiting. It however produced slight vomiting, but it troubled me very little. I afterwards took the extract in doses of one-quarter of a grain every three hours for three days. There was a feeling of clumsiness in the limbs produced, and several times the peculiar muscular affection which I mentioned above. It seemed to affect indiscriminately those muscles that had been most fatigued for a short time before, being sometimes the calf of the leg, sometimes the forearm, sometimes the extensors of the knee. The feeling of contraction at the stomach was occasionally perceived, and I thought there was some increase of urine, but am not positive.

A gentleman of ordinary susceptibility, as I judge, took two grains of this extract, which produced all the effects experienced by myself, but in a much greater degree. Violent vomiting, long continued, with great pain and prostration. He took over ℥iij. of laudanum without much effect, till he took two pretty stiff doses of warm brandy, which soon relieved him. Another of my acquaintance, a man of very susceptible habit, took either one-quarter or one-half grain, he could not tell which, as it was done up in one-half grain and one-quarter grain papers, which was followed by considerable bad feeling at the stomach for some time.

Those who have used this article, consider it as very like Sanguinaria Canadensis in its medicinal properties, and that either may be used as a substitute for the other. The Kalmia is much slower in operation than the Veratrum. It is also more narcotic in proportion to its emetic power. The peculiar sensation of fatigue in the muscles, which for a long time precedes vomiting from Kalmia, is not felt till afterwards in the use of the Veratrum, and is less conspicuous. The peculiar pricking sensation in the skin is also less conspicuous. It was quite cold weather when I took the Veratrum, and I cannot say as to its diaphoretic power. The sensation of cramp in muscles, while yet they are uncontracted and almost paralytic, is peculiar to the Veratrum. Both vomit in precisely the same way, and produce precisely the same feelings and the same effect on the pulse and sight. As the effect of Kalmia is longer coming on, it is longer going off when left to itself. The Kalmia has considerable soporific power, which the Veratrum does not seem to possess directly. The Veratrum is known to be a powerful deobstruent; and from the effect of Kalmia in curing rheumatism, and from the similarity of its effects, when taken by a healthy person, to those of the Veratrum, I am confident that it is more or less deobstruent. All the narcotics that I know of, that are emetic as a primary part of their operation, are also deobstruent. If the Kalmia should not be found to be deobstruent, still as a narcotic I think it will be valuable. From its great effect, when given short of vomiting, in diminishing frequency of pulse, I think it might be of benefit in cases of irritative frequency of pulse in which Digitalis is used. In cases of fever, with dry skin, much restlessness, and quick pulse, where the grade of action is neither materially atonic or entonic, I think it worth trying. In chronic diseases, attended with such irritation, especially in cough, with quick pulse and tendency to phthisis, I should think well of it, though it should prove only narcotic. The length of time it takes to get under its full influence, would be something against it in very acute disease. In mania furibunda, where large quantities of narcotics are wanted to quiet the patient; and in epilepsy, used as Stramonium is used, with some metallic tonic, I think it worth trying. In case it should prove deobstruent, as I am confident it will, it will be appropriate in the same cases as those in which Actæa, Sanguinaria, &c. are, except that they will have the advantage in very acute cases.

I think that the vegetable deobstruents will be found to answer the purpose for which mercury is used in our ordinary fevers, as an alterative or deobstruent. It is true all deobstruents are not alike, and one may be more appropriate to one form of diseased action than another, which is yet to be preferred in another case. I have heard of some few cases in which they have been used in common fever of the synochous type, in which, by the account, the patients did as well as when treated according to the more common plans. The Thompsons, who treat everything by lobelia and steam, have considerable success in fevers. This I am informed by a friend who had something to do with the Thompsons not long since, but who has since then become a student in medicine of a different school from the Thompsonian. In some of the phlogoticæ, especially in the pneumonitides, they are found to be fully equal to mercury, and even, in the opinion of many, superior. It is more common to join them with mercury in the treatment of these affections, than to use them alone.

I have used the Kalmia in but two cases of disease, and neither of them were very favorable cases for its action, and I can therefore say nothing decided from them. The first was a case of rheumatalgic affection of the intercostal muscles, well characterized by much cough by turns, little expectoration, tenderness of the intercostal spaces, but no swelling; pain severe, but remitting, and worst at night; chest sounding clear on percussion, and the pain changing its seat to different parts of the chest, and finally to the abdomen—apparently at one time affecting the stomach, and leaving the chest entirely. The pulse was very weak and rather slow, and the skin covered with clammy sweat. I directed wine in a moderate quantity, and gave ʒ ij. and afterwards ʒ iij. of tincture of Kalmia every three hours, with fifteen or twenty drops of laudanum. The specific effect of Kalmia in producing vomiting was produced in two or three days, and the patient was apparently better for a time; but whether it was anything more than an ordinary remission of the disease, I cannot tell. At any rate, the amendment was transient. The patient was evidently too low. I then put him upon Fowler's mineral solution three drops, tinct. lyttæ twelve, laudanum fifteen, and tinct. guiac. something more than ʒ ss. every three hours, with increase of wine, under which he improved so rapidly as to be able to do the duty of a nurse on the 4th day, though still weak.—The other case was a sort of rheumatalgico-neuralgic affection of the sciatic nerve, attended with a troublesome cough. The patient got better of both under its use, but not rapidly, though I think it had some effect in the case. The patient was a girl of 12 years of age, of a very susceptible habit. She could take ʒiss. of the tincture four times a day, but ʒij. made her dizzy. I am, &c. X+Z.

EXTRAORDINARY CASE OF LODGMENT OF A FOREIGN SUBSTANCE IN THE LOWER PORTION OF THE COLON.

REPORTED BY GEORGE MOODIE, M.D.

[We are indebted to our friend Dr. Channing, of this city, for the following remarkable history, which was sent him for publication in some medical journal. We shall take the liberty to place after it a brief discussion that lately occurred in the Medical Society of London, on a case bearing some resemblance to that of Dr. Moodie.]

I was called, on the fifth of April, 1834, to visit a man named Steven Hovey, in North Andover, in the vicinity of the factories. I inquired of the person who came for me, the nature of the case. He could give me no further information than that the patient had a pain in the abdomen, and complained of numbness in one of his legs, and requested me to make haste. I hurried as expeditiously as possible to the patient's residence, supposing it to be a case of common colic. On my arrival at the house, I found the patient up, with his clothes on, exhibiting but slight marks of indisposition. I immediately asked him if he had an attack of colic. He hesitated a moment, and then said, looking round to see if any one was within hearing, "I have got a tin tumbler up in my body! and I wish you to keep the circumstance as private as possible, as people will laugh at me." I immediately inquired how he could possibly get it there, and how long it had been there. He simply stated that it had been in his bowels about thirty hours; that he had had a prolapsus of the rectum (an affection to which he had been subject), for which I had treated him in 1829, but he had not been troubled with it since, till the present time, when, owing to unusual dilatation of the sphincter ani, the rectum had descended to a considerable extent, and having tried the usual method of reduction with his fingers, &c. ineffectually, he placed a large tin tumbler on the floor and sat upon it, thus forcing up the prolapsus. When the intestine returned to its proper situation, owing to a sudden and violent action of the sphincter and the sudden and quick contraction of the parts, the tumbler passed up with the rectum. He had made an ineffectual effort to extract it with his fingers; he then had recourse to a pair of shoemaker's forceps. With these he had considerably broken and flattened the edge of the base, or rim, of the tumbler, and forced it beyond the rectum, into the colon. This was the situation in which I found it on my arrival.

After examination, considering it as nearly a hopeless case, I immediately placed him in a horizontal position, and passing my hand and forearm through the rectum, seized the substance and made a powerful effort to extract it; but owing to the violent contraction of the walls of the rectum and the sphincter, I had not strength to accomplish my object. I then desisted, and had recourse to the blunt hook. With this I brought it down so as easily to distinguish it externally; but owing to its flattened state, it hitched in the plicæ of the intestine. Finding I had not strength to extract it, if it was practicable, and wanting advice, the Hon. Jos. Kittredge was called. He made another attempt to extract it with the blunt hook, but did not succeed; he likewise said he should like more counsel, and the Haverhill physicians were sent to. Accordingly, about half past two or three o'clock, Dr. Whiting arrived from Haverhill. I then related the facts of the case as they had come within my knowledge; stated that I thought the sphincter ani might be divided, and an incision made up the sciatic notch in the part nearest the sacrum, the walls of the rectum divided by a bistoury, and the levator ani cut through, a speculum ani introduced, and the parts dilated so as possibly to extract the offending substance. I thus stated my opinion, and submitted myself, as the youngest, to be guided by my elder associates. Dr. Whiting was of opinion it was best to extract it with several kinds of forceps, and accordingly the patient was placed again in a horizontal position. Another young man and myself dilated the parts, and the tumbler was brought down and seized with forceps by Drs. Kittredge and Whiting, and they made a powerful but vain effort to extract it. It was then agreed that Dr. Whiting, he having the smallest hand, should introduce it beyond the rectum, and make another effort to bring down the tumbler; he succeeded in passing his hand into the rectum, but was unable to bring the tumbler away, owing to its lodging in the folds of the intestines. The patient now expressed a desire to be opened through the walls of the abdomen; this, he was told, would produce certain death. It was then proposed to open through the levator ani, but Dr. Kittredge thought he might expire from hæmorrhage from the hæmorrhoidal arteries; and as it was now growing dark, the operation was abandoned.

The patient lived about three days after this. His tongue sloughed and turned black, and gangrene took place in the large intestines. The tumbler was extracted after death; it measured 3 1-2 inches in length, 3 1-2 in width in the direction of the flattened part, 2 inches across the bottom, and would hold nearly three gills.

Hovey had been cut for the stone in the Massachusetts Hospital in 1828, by Dr. Warren. It seems that he introduced the nucleus of the stone himself through the urethra, which consisted of a small bean, when a boy; it rolled about in the bladder, and this collected the stone which was extracted by Dr. Warren.

I have been requested by the neighboring faculty to prepare this statement—

1. Because it is a new and unheard-of fact in the annals of medicine.
2. To prevent misrepresentation.
3. To show how large a substance may pass through the rectum.
4. To turn the attention of the faculty to the proper method of treatment, should like cases ever occur.

Medical Inquiries arising from this Case.

1. What analogy would an operation in this case bear to the operation for fistula in ano?

2. If an incision had been made, how far could it be carried without endangering the nerve ?

3. How much dilatation would be obtained by dividing the sphincter ani ?

Boxford, Mass., *April* 18, 1834.

A Case in which a common Earthenware Egg-cup was found in the small Intestines.—Mr. Dendy brought before the notice of the Medical Society of London, the following singular case :—A man, aged 60, came into the Christ church work-house, with scrotal hernia, which had existed thirty-five years, but was partially reducible. He had been for some time subject to repeated attacks of chronic diarrhœa and dysentery, and ten weeks previous to his death he had diffused peritonitis. About three weeks previous to that event he was seized with stercoraceous vomiting, and the taxis was applied, but was not perfectly successful, as a small tumor still remained, similar in appearance to a knuckle of intestine. The symptoms continued, and on the 4th of December he died.

About twelve hours after death, Mr. Dendy, in the presence of Mr. Stevens and Mr. Brown, proceeded to examine the body. The stomach appeared to have suffered from inflammation, and the pyloric orifice could be distended with the greatest facility ; the small intestines were matted together, and their coats were so attenuated that they formed a perfectly diaphanous membrane. The ileum was of a purple color, and marked in some places with little patches of ulceration ; in the interior of this intestine, about ten inches from the ileo-colic valve, was found a common-sized earthenware egg-cup, resting upon the lumbar vertebræ, near the posterior superior part of the crest of the ileum ; the mouth of the cup was in the direction onwards, towards the large intestines, and its interior was stained of a black color. No portion of the intestine was found in the hernial sac, but there was a chocolate-colored fluid, similar in appearance to decomposed intestine, in it. The ileo-colic valve was perfectly healthy, and of its natural size ; and, although the colon and rectum were traced throughout their whole course, no marks of disease could be discovered ; the cœcum was found full of scybala. The deceased had been much addicted to drinking, but had never exhibited any symptoms of insanity : nor did Mr. Dendy, from any part of the man's conduct, although he had been under his observation some time, expect to find such a source of disease. He was of opinion that, from the healthy state of the large intestines and the valve, and the diseased condition of the smaller ones, the cup must have passed by the mouth. The portion of intestine which had formed the hernia was below the cup.

Mr. Salmon could not think that this body had passed the pylorus ; it would probably have suffocated the man. He had, however, seen many instances where patients had confessed to having passed foreign substances through the anus : it might by possibility pass the valve of the colon, but could not, in his opinion, pass by the mouth.

Mr. Stevens thought that if the cup had entered into the stomach, it might with facility pass the pyloric valve, which was so unusually large : the state of the intestine led him to suppose that it had passed in this way.

Mr. Hooper mentioned a case which occurred at St. Bartholomew's Hospital, in which a six-ounce bottle had been passed into the rectum. Mr. Lawrence was sent for, and on his arrival proceeded to dilate the anus with his fingers, and finally succeeded in extracting the foreign body.

Dr. Ryan remarked, that if this man were subject to delirium tremens or melancholy, he considered it probable that he might have swallowed the egg-cup when his mind was much affected. He mentioned the circumstance of Gosse, of Geneva, having swallowed metallic balls of two inches and a half in diameter.

Mr. Headland made some remarks as to the fact that the egg-cup was not acted on by the gastric fluid.

Dr. Severn observed that the cup was made of silex, which was not soluble in the strongest acids, and it was then glazed with cobalt with the same intention.

Mr. Kingdon said that he had seen a man nearly killed by swallowing a shilling, but that eventually it passed by the rectum.

Dr. Ryan, Mr. Hooper, Mr. Dendy and Mr. Salmon, mentioned cases in which farthings, half-pence, pence, and sovereigns, were swallowed without having produced any bad effect.

Several other members spoke as to the probability or improbability of the cup having passed by the mouth.

INTRODUCTION OF AIR INTO THE VEINS.

READ BEFORE THE BOSTON SOCIETY FOR MEDICAL IMPROVEMENT.

BY BENJAMIN F. WING, M.D.

[Communicated for the Boston Medical and Surgical Journal.]

Gentlemen,—I have selected for the subject of my communication this evening, the Introduction of Air into the Veins. My attention was first drawn particularly to the subject, in consequence of having witnessed the death of an individual, which was supposed to have been caused by this accident while she was undergoing a surgical operation. Believing that as good an understanding of it might be obtained by interrogating nature, through a series of experiments, as by studying the different authors who have written upon it, I undertook the following experiments.

Exp. I.—The external jugular vein of a full-sized rabbit was laid bare for some distance, and a branch of it selected for the insertion of a tube, that the current of blood through the principal vein should not be interrupted. By means of a small syringe the air was forced into the vein, and globule after globule was seen to enter the jugular and pass along with the blood, producing in the vicinity of the heart a slight gurgling noise. This organ immediately testified its presence by violent action for the space of about one minute, when it suddenly ceased to act. The animal made a few convulsive struggles, uttered a cry, gasped at intervals of some seconds, and expired.

An examination of the body was made twenty minutes after death. The muscles contracted under the stimulus of the knife. The brain presented no uncommon appearance. Its bloodvessels were not engorged, nor was any air discovered in them. The lungs were of their ordinary appearance, filling the cavity of the thorax, but immediately collapsing on the admission of air. On laying open the pericardium, the coronary vessels of the heart were perceived much injected. The right auricle and ventricle, together with the venæ cavæ, were distended with air mingled with blood in a semi-coagulated state. In the left auricle was found a little blood with a small portion of air. The corresponding ventricle was entirely empty.

The six succeeding experiments were tried upon other rabbits, varying the quantity of air from a volume equal to one, to that of three fluid drachms. The results being similar to the one described, it appears unnecessary to state the particulars attending each case. I shall therefore pass on to the relation of some that were tried upon larger animals, where the quantity of air in proportion to their size could be more easily regulated.

Exp. VIII.—Air was gradually thrown into the jugular vein of a small-sized sheep, in a manner similar to that described in Experiment I., until the quantity equaled the volume of a fluid ounce. The time occupied did not vary far from ten minutes. The gurgling noise noticed in the first experiment, slight difficulty of respiration, together with gentle struggles indicative of suffering, followed each movement of the piston forcing the air into the vein. After these symptoms subsided, the vein was secured and the animal released. It immediately manifested a disposition to eat, sought its fellows, and mingled with the flock.

Exp. IX.—In this experiment, double the quantity of air was used. The symptoms differed only in intensity. They speedily subsided, and the sheep, after being released, could not be distinguished by any peculiarity of manner.

Exp. X.—A volume of air equal to three fluid ounces was injected with such rapidity, that globules were seen constantly passing through the jugular vein towards the heart. In this case much disturbance occurred. The animal evinced much suffering, the struggles approached convulsions, and the respiration became very difficult. After an interval of fifteen or twenty minutes of doubt as to the result, the symptoms gradually subsided. The sheep, when loosed from its bonds, could scarce support itself, but at length it walked away.

On the following day the three sheep subjected to experiments presented no peculiarity of manner. One was slaughtered in the usual way. No air was seen to escape with the blood, nor was any coagulum found in the heart. The lungs were of their ordinary appearance. In fact, I could not discover any difference between those that were subjected to the experiments and those that were not.

Exp. XI.—In this instance the precise quantity of air could not be determined. It amounted at least to a volume equal to six fluid ounces. It was injected as expeditiously as possible, and its effects were immediately apparent. The heart palpitated violently a short time, then suddenly ceased to convey any sound to the ear; convulsions and gaspings were soon followed by a suspension of all signs of animation. The jugular vein that was dissected of its integuments became enormously distended, and the contents apparently stagnant. Under the conviction that the animal had become a victim to the experiment, the jugular was freely opened, and blood mingled with air was freely discharged. After a considerable quantity was evacuated, the sheep again respired, and the heart resumed its action. It was finally killed by opening the carotid artery. In this case some frothy blood was found in the right side of the heart, and the muscles presented an unusual redness, in consequence of a retention of blood in the capillary vessels.

The result of the eighth, ninth, and tenth experiments, show that air when admitted into the veins, although producing derangement in the functions, yet does not necessarily cause immediate death; but that the symptoms will gradually subside, and the organs resume their ordinary functions, provided the quantity is not too great in proportion to the size of the animal. But when this is carried beyond a certain point, the seven first experiments show as clearly that death will ensue. The last experiment may be considered doubtful in its effects; still, until the opening of the jugular, the symptoms were as strongly marked as in the cases where death was undoubtedly caused by the air.

By reference to these experiments it will be seen, as would naturally be inferred, that the first evidence of the presence of air is disturbance in the heart, increasing until its action is entirely suspended,—not by a gradual diminution of its power, rendering the pulsations weaker and weaker, but suddenly stopped in its high tone of action. Difficult respiration succeeds the tumultuous action of the heart, and increases to gasping at lengthened intervals, until this function also ceases. The muscular system is, to all appearance, at first only excited to increased action by the pain endured, nor does this action differ from that which is testified by the animal when an incision is made preparatory to the experiment, until the animal approaches the agonies of death, when it becomes universally spasmodic.

Is it necessary for the explanation of these vital phenomena, to suppose that the air arrives at each organ before any aberration of function is manifested—or cannot they be more satisfactorily accounted for, by referring the first impression to the heart, where we know the air arrives, and the other effects to the functional dependence or sympathetic relation that exists between the heart, lungs, and brain? Can we not suppose that the air impairs the power of circulation, first by distending the heart with its own volume, and secondly by causing an imperfect closure of the valves, and thus permitting a reflux of blood at each contraction of the ventricle, which causes in its turn an increased disturbance, until it goes beyond the power of reaction to overcome?

I am aware the subject deserves a more varied and extensive course of experiments; but as circumstances do not permit me to continue my researches at present, I must beg leave to offer you these first fruits of my labor.

Boston, May, 1834.

BOSTON MEDICAL AND SURGICAL JOURNAL.

BOSTON, MAY 14, 1834.

ON THE EXISTENCE OF PAIN AFTER DECAPITATION BY THE GUILLOTINE.

The humanity of the mode of death practised by the French in their public executions, has long been a subject of doubt and of discussion. The belief of the great majority of those who have reflected on this interesting and important question, probably is, that all sensation terminates at the instant of decapitation. This certainly was the ground taken by Guillotine, the author of this mode of punishment, and which has been supported by Petit, Cabanis, and other physiologists. Many have thought that suffering ceases instantly in the *body*, but continues in the *head* until its vital heat is extinguished; and the late Dugald Stuart, in conversing on this subject, told us that a distinguished philosopher once maintained to him that the guillotine is a most cruel instrument of death, inasmuch as it created a double agony, one of the head and one of the body, both of which continue for several minutes. This latter opinion has derived a color of confirmation from the well-known phenomena succeeding decapitation in birds and quadrupeds, and has been maintained by Soemmering, Sue, Mojon, Castel, Aldini, and other able physiologists. Some of the facts on which this opinion is based, were recently related to the French Institute by M. Julia Fontenelle—and they are worth recording.

"1. Sue cut off the head of a turkey, and the animal had all its movements for a minute and a half; the bill and pupil moved strongly. The body, which had been motionless for a minute and a half, rose, walked, shook its wings, guided one of its claws to its neck, and died at the end of six minutes. Another turkey exhibited the same phenomena.

2. After the decollation of a sheep, the head preserved all its movements for two minutes, and the body for twelve minutes. The movements were so strong as to require three men to master them. This experiment was repeated by the author with a similar result.

3. The section of the head of a calf being made, the head presented well-marked movements of the eyelids, pupils, ears, nostrils, muscles of the face and lips, for six minutes and a half. The body continued to move for seven minutes. An expression of the most agonizing pain was depicted on the different parts of the head.

4. M. Mojon, in company with MM. Guillotine, Aldini, and Nauche, made some experiments on the guillotined bodies in 1804. The results were, that, during a quarter of an hour after decollation, the head being exposed to solar light, the raised eyelids immediately closed; that the head of the decapitated criminal was sensible to stimulants; that the tongue, drawn out of the mouth and pricked with a needle, retracted, and the features of the face indicated a painful sensation; and that the organ of hearing perceived sounds. The author asseverates that he saw the head of a guillotined subject turn its eyes towards the side whence it was called.

5. Weciard saw the lips of a man's head, that had just been cut off, move.

6. The author of the *Connaissance des Bêtes* assures us that the head of a decollated subject turns its eyes to express pain; that it moves its lips and bites the ground; the heart meantime continuing to beat for a few instants.

7. Doctor Castel maintained that the head, after decapitation, is capable of sensation, and that this sensation is more rapidly extinguished in it than in the trunk. M. Julia Fontenelle does not agree to this. According to him, after decollation, the head preserves for some minutes the arterial blood sent to it by the carotid and vertebral arteries. As soon as a section of them is made, they contract, diminish in calibre, and, while this spasmodic state exists, but very little blood escapes from them. The contrary takes place in the trunk; the heart, continuing its systolic and diastolic movements, drives four jets of blood through the cut arteries during its action; in consequence of which the body becomes more quickly cooled than the head. If we reflect on the anatomy of the brain, and particularly on the distribution of the arteries and veins in it,—the arteries going to it in a curved manner, being infinitely divided, having extraordinary connections with veins and sinuses; the veins going from it in a tortuous manner, and the whole conspiring to retain the blood in the brain; it is easy to perceive that the phenomena dependent on the presence of blood in that organ would continue therein so long as the vital force or influence existed there, that is, until the dissipation of all vital heat. M. Julia Fontenelle then examined the researches of Cotugno, Egel, Galvani, Gren, Hunter, Hermestaedt, Jacquin, Guhtenberg, Moll, Shærer, Valli, Vassali, &c. into the decollation of man, gallinaceæ, batraciæ, and the inferior animals, and finally drew the following conclusions therefrom:—1. That, after decapitation, pain is prolonged for some time. 2. That the head retains feeling nearly until the extinction of vital heat. 3. That death by the guillotine is one of the most painful known."

Now we published, three weeks ago, a case by our esteemed correspondent, Dr. Spencer of Ticonderoga, which affords more true light on this question, than any of the facts related by M. Fontenelle,—a case which is here the better evidence, inasmuch as it is related without reference to this question. It is briefly this:—

" E. D., aged 50, a man of hale constitution and robust, in making an effort to scale a board fence, was suddenly precipitated backwards to the ground; striking first upon the superior and anterior portion of the head, which luxated the dentatus anteriorly on the third cervical vertebra. He was at length discovered, and taken in (as the patient said) after he had lain nearly an hour, in a condition perfectly bereft of voluntary motion; but being present I did not even suspect that the power of sensation was also gone, until the patient (whose speech remained almost or quite perfect, and who was uncommonly loquacious at that time) said, did he not know to the contrary, he should think he had no body. His flesh was then punctured, and sometimes deeply—even from the feet to the neck; but the patient gave no evidence of feeling, and when interrogated, answered that he felt nothing; 'and,' added he, 'I never was more perfectly free from pain in my life,' but he remarked that he could not live, and accordingly sent for his family, twelve miles distant, and arranged all his various concerns in perfectly a sane manner.

The head was thrown back in such a position as to forbid his seeing his body. The pulse were much more sluggish than natural. Respiration and speech but slightly affected, but were gradually failing; but he could articulate distinctly until within a few minutes before his death. All the senses of the head remained quite perfect to the last. He died forty-eight hours after the fall.

Repeated attempts were made to reduce the dislocation, but the transverse processes had become so interlocked that every effort proved abortive. There was, undoubtedly, in this case, a perfect compression of the spinal marrow, which prevented the egress of nervous influence from the brain, while the pneumo-gastric nerve remained unembarrassed."

This case appears to us to prove conclusively that no agony, no suffering, no sensation whatever, can exist in the *body*, after its connection with the brain is dissolved.

It teaches us, further, something respecting the state of the *head* after decollation. The state of this part must depend on two conditions—one the condition of the brain and nervous system, the other the condition of the vascular system. So far as the former is concerned, it shows that sensation *does* remain in the head after the spinal cord is divided, even high up in the neck. Although a "perfect compression" of this cord existed, yet "all the senses of the head remained quite perfect to the last." So far Dr. Spencer's history is analogous to an instance of decapitation, and is admirably adapted to enlighten us on the question in dispute. But there is one essential point of difference in the two cases, which may be found in the state of the vascular system. In the one case the circulation of the blood in the brain is not essentially impeded, whilst in the other it is speedily at an end. We know that all sensation must cease on the evacuation of the cerebral bloodvessels; but Dr. Spencer's case teaches us, that, so long as the blood continues to pass through the brain, so long is there sensation in the head, although its nervous connections with the body be interrupted. After decapitation, therefore, by the guillotine, the sensation of the head probably remains until its vessels are evacuated.

If this view of the subject be correct, it becomes an interesting inquiry how long the head preserves its arterial blood after decollation: in what degree this term is protracted by the spasmodic action consequent on the section; and whether the death of the head may not be rendered more speedy by maintaining it in such a position as shall allow the force of gravity to aid in freeing it of its blood. If these views are correct, it would seem that death by the guillotine, as usually practised, is not quite so instantaneous as has been generally supposed, and that that mode of execution is most consistent with the dictates of humanity, which most instantly arrests the circulation of the blood in the brain, rather than that which most quickly severs the spinal cord.

INTRA-AURICULAR LITHOTRITY.

The following singular operation for extracting a foreign body from the ear, we obtain from the Journal de Therapeutique. It is there termed *Intra-Auricular Lithotrity.*

In the month of July last, a child aged seven years, in playing with other children, let fall a cherry stone into the passage of the ear. A month after the child was presented at M. Dupuytren's consultation; the exploration of the ear by daylight (at sunrise) and examination by catheter, proved the existence of a foreign body at the bottom of the meatus.

It may easily be conceived how important it was to extract the body in the shortest possible time, to prevent mortal suppurations within the cranium, and various other possible accidents. The attempt was first made, but in vain, to extract the foreign substance with several kinds of forceps. These instruments could take no hold, in consequence of the close apposition of the foreign body with the undilatable sides of the canal. M. Dupuytren then came to the conclusion that the stone must be broken previously to extraction. A kind of metallic drill, sheathed in a silver canula, was made for this express purpose by Charrière. This instrument is a true lithotritor, enclosed in a simple canula without forceps. The head of the child having been inclined toward the opposite ear, and placed on a table, where several assistants retained it firmly, the instrument was introduced, closed, to the stone, then the sheath being withdrawn a little, the drill was made to act on the stone, with great caution, to avoid the neighboring parts. After a few turns of the instrument the stone remained engaged at the end like a cork caught within a bottle ; but it was impossible to withdraw it whole, for on the one hand the inflammation had diminished the calibre of the passage, and, on the other, the size of the stone itself had augmented since its introduction. The instrument was, therefore, removed, and a larger drill ordered, by the aid of which the stone was broken into several fragments ; its extraction was then effected partly with the forceps, and partly by consecutive inflammation.

NEW PROFESSORSHIP IN THE MASS. MEDICAL SCHOOL.

It has long been a desideratum that the duties of the present Professor of Anatomy and Surgery in our Medical College, which are too onerous for any one person, should be shared by another. We have now the pleasure of announcing that such an arrangement has been made. The course on Anatomy and Operative Surgery will be given as heretofore, by Dr. Warren, and Dr. George Hayward will give a separate course on the Principles of Surgery and Clinical Surgery, for which it is hoped and expected a distinct professorship will shortly be created by the government of the University.

We are indebted for several Communications, which shall be published with as little delay as possible.

Whole number of deaths in Boston for the week ending May 10, 19. Males, 5—Females, 14.

Of consumption, 6—dropsy on the brain, 2—convulsions, 1—erysipelas, 2—child bed, 1—inflammation of the bowels, 1—croup, 1—infantile, 1—dropsy, 1—liver complaint, 1. Stillborn, 4.

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THE BOSTON MEDICAL AND SURGICAL JOURNAL is published every Wednesday, by D. CLAPP, JR. at 184 Washington Street, corner of Franklin Street, to whom all communications must be addressed, *post-paid*. It is also published in Monthly Parts, on the 1st of every month, each Part containing the weekly numbers of the preceding month, stitched in a cover.—Price $3,00 a year in advance, $3,50 after three months, and $4,00 if not paid within the year.—Every seventh copy, *gratis*.—Postage the same as for a newspaper.